exact role of the procedure in our armamentarium I believe most physicians feel is still to be defined. Currently, in the hands of a limited number of endoscopists in this country who are expert in this technique, endoscopic ..ansphincter stone removal has been useful. As experience with the technique increases, its indications as well as its hazards will be defined.

Incidentally, I might also take this opportunity to question the statement in Dr. White's editorial comment which accompanied our panel discussion. He assumed that if 600,000 cholecystectomies were carried out each year in the United States, there would be 30,000 patients left with retained stones rather than the 3,000 to 4,000 we had proposed.

According to the National Center for Health Statistics[1] there were 402,000 cholecystectomies done in the United States in 1974. The true incidence of retained stones following cholecystectomy is difficult to establish, and most of the figures available pertain to years during which operative cholangiograms were used infrequently and choledoscopy not at all, and many patients were operated upon late in the course of their disease when the incidence of stones in the common bile duct was increased.

Recent studies all show a pronounced decrease in the incidence of retained stones. Our estimate of the number of retained stones following cholecystectomy was based on the assumption that 20 percent of the 400,000 cholecystectomies would be accompanied by common bile duct exploration and that retained stones might occur in 3 percent to 5 percent of the patients so explored. Thus it would seem that our guess of 3,000 to 4,000 retained stones might be somewhat more realistic today than the 30,000 proposed by Dr. White.

WILLIAM P. LONGMIRE, JR, MD
Professor of Surgery
University of California, Los Angeles School of Medicine
Los Angeles

REFERENCE

1. Utilization of Short Stay Hospitals—Annual Summary for the US, National Center for Health Statistics. US Dept of Health, Education, and Welfare, 1974

Evidence in Medicine as a Natural Science

To the Editor: In medicine, nowadays, the rule of evidence for the acceptance of new knowledge of a concept, a therapeutic procedure or a medication for a specific ailment is the prospective double-blind randomized study (PDBRS). Historical controls, pilot studies, preliminary information and, even less, promising results smack of inconsistency and prematurity and therefore are looked at with a critical eye, a skeptical mind or at least, like the twist of the pretzel, they are to be taken with a grain of salt.

Speculation by an experimentalist is to be avoided as a forbidden path, as foregone or analogistic conclusions, beyond the probabilities of random sampling. It is rightfully so in the simple logic of common thought processes, for the roads of speculation (literally mirror-image, that is virtual, unreal) are paved with refractive potholes and parallactic false steps.

A fundamental difference exists, however, between speculating and theorizing, between speculation and theory. While speculating is extrapolating beyond the limits of the physical evidence, theorizing is actually experimenting with previous physical evidence not at the laboratory bench but in the laboratory of the mind.

For those so endowed it is possible to leap over and bridge across the same path of double-blind or foolproof experimentation and seize the evidence by the process of creative association. Their discoveries precede further experimentation rather than follow it. They anticipate by creative deduction rather than discover through experiment. So it was, as good examples, for Einstein and for Harvey as for Medawar and Burnet. Yet their evidence is only accepted after experimental confirmation; this is not as much for the sake of the discoverer (for, as Guy, once so confronted, only said "but I already knew it") as it is for the sake of those who can only accept it as an experimental fact.

The advent of direct observation by Galileo and the experimental method by Claude Bernard in natural sciences has brought down to larger numbers of prepared persons the ability to gather scientific evidence (hence called scientists) much as serial industrial manufacturing provided to the masses products that before were only available to the privileged wealthy.

But because the experimenter so contributes to the vastness of scientific discoveries, it must not be overlooked that today, and henceforth, he will overlap with true theoreticians. These must be distinguished from the quickstep observer since no one has yet invented an "inferometer." The experimenter is forever uncertain, the theoretician knows. The experimenter is analytical, he has to deal with all the uncertainties and variables of the parameters and measurements of the system he sets in motion and draw tentatively the fleeting evidence from it. The theoretician is a synthesizer, evidence comes to him with a certainty that, though Heisenbergian, is only limited by the dimensions and forms of the universe within which he is working. Euclid is right on his two to three dimensions as Einstein is on four or five, and so on. The Euclidean view was necessary but not sufficient. Where Euclid felt secure in the simple beauty and *superficial* logic of planar geometry, Einstein foresaw the limitations of even his generalized relativity and searched continuously for further unification.

A distinction must be made between unproved experimental observation, often erroneously called theory, and *theoria vera,* the result of veridical and integral observations by the mind. The example given of Medawar and Burnet illustrates the many that could be quoted of advances in biology and medicine by experimenting in the mind. Tyler, for instance, predicted that backgrafted F_1 hybrids would develop lymphomas. As basic data of first order become available from partial and limited syntheses of function-structure phenomenology, the minds of the real theoreticians will transduce and translate them into higher, and yet simpler, forms of synthesis-comprehending.

There is no precision and exactness in scientific evidence. Its median, focal, photographic clarity, like the insisting thematic leitmotiv of a Beethoven symphony or the immediacy of a Flemish portrait, are no more true (or more beautiful) than the indistinctness of impressionistic painting and music and even distorting and less tonal followers. All are part of a Poisson curve, a Dalton-Gaussian distribution, nature's trials and errors, a quantum, discontinued nature, all wave-matter, time-space. Only the human mind is able to integrate it into abstract thought of continuity. The experimental observations close in a polyhedron whereas the theoretical thought finishes it to a sphere.

In medicine, with its inherent pragmatism, we go by what works, what time tests and sanctions, what gets wide acceptance, what is well remembered and even fashionable. In narrower circles one even speaks of standards of community practice. PDBRS are relatively recent and exist only for a very limited number of concepts, procedures and medications. Medicine cannot stop to take a total PDBRS inventory of its knowledge stock. Such inventory would be irrelevant because the precision of its discoveries lies not in whether they are PDBRS-proof but in the concatenation of factual data, that is, in their cross-veracity. This cross-veracity is what appears as a sudden, thrilling, illuminating evidence and inner, unconfessed, certainty to its discoverer and less, sometimes never, to its critics (even the constructive ones), until it is *released* once again in another equaled mind.

SERGIO DeCARVALHO, MD, PhD
Bellflower, California

A Technique to Prevent Headaches After Diagnostic Lumbar Puncture

TO THE EDITOR: Lumbar punctures for diagnostic examination of cerebrospinal fluid (CSF) have been carried out since the turn of the century and have frequently been accompanied by post-lumbar-puncture headaches (PLPH).[1]

Tourtellotte[2] has reported in his summary of 21,000 standard lumbar punctures that the incidence of PLPH is 32 percent for diagnostic lumbar punctures, 18 percent for obstetrical spinal anesthesia and 13 percent for nonobstetrical anesthesia. The typical PLPH[2] starts about 10 hours after the lumbar puncture with a backache, followed within 14 hours by a postdural headache. The range of onset is from 15 minutes to four days after the procedure. It persists for 2 to 14 days (average 3 days) and is characterized by a retro-orbital frontal aching and pounding. Occasionally, it is accompanied by nausea and vertigo. Immediate relief usually occurs when the person lies down. Few people are able to carry out normal activities with such a headache and analgesics are of little use. The most effective treatment is the epidural blood patch recently reviewed in this journal by Brodsky.[3] Of 570 patients with PLPH, 95 percent had relief after this procedure. Many patients are subjected to risk of PLPH, since more than 800,000 lumbar puncture trays are used each year in nonfederal and non-state-operated hospitals (unpublished data provided by IMS-America, Ambler, Pennsylvania 19002, 1978).

Although the PLPH can be effectively treated, it is preferable to prevent them from occurring. In most instances this can be done by using a thinner needle than is standard. The single factor that contributes most to the development of PLPH is the diameter of the hole made in the dura by the needle.[2,4] The use of the prone position for 1 to 24 hours following the procedure to prevent